which touches the surface at the first corner of the quadrilateral."

More generally the theorem here referred to shews that for the inscribed quadrilateral we may substitute a gauche polygon with any even number, $2n$, of sides; and for the nonagon, another gauche polygon, with $4n + 1$ sides, connected with that polygon of $2n$ sides, by the same law of construction as that which had connected the nonagon with the quadrilateral; and that then the tangent plane to the surface at the first corner of the polygon of $2n$ sides, will be parallel to the plane through the first, middle, and last corners $(1, 2n + 1$ $4n + 1)$ of the polygon of $4n + 1$ sides.

The Secretary presented, from the Rev. W. C. Armstrong, an earthen sepulchral urn, found on the 17th of March last, in a field, part of Moydow glebe, county Longford; together with the jaw bone of a man whose skeleton was found near the urn. He also presented, from Mr. R. Hitchcock, a large stone-hammer, used by miners, and found near Killarney.

Several donations of books were also made to the Library, which will be found noticed in the Appendix.

Thanks were returned to the several donors.

APRIL 23RD, 1849.

SIR WILLIAM BETHAM in the Chair.

DR. Harvey made a communication respecting the nature of the Fructification of the Rhodospermatous Algæ.

"In the Rhodospermatous Algæ, or Florideæ, the fructification presents itself under two forms. *Two* sorts of reproductive bodies are produced by each species of these algæ, both sorts equally capable of germinating into a new plant, both, therefore, performing the functions of a *seed*. These repro-

ductive bodies are never found together on the same individual, but some individuals produce one kind of fruit, some the other kind. One sort is called a *spore*, the other a *tetraspore*, because it divides at maturity into four parts or *sporules*.

" It is not reasonable to regard both these bodies as *true seeds*, because they are formed in very different manners, and because it is against the analogy of the rest of the vegetable kingdom to admit a *double* seminal system. Other plants have but *one* kind of SEED proper to each species. But other plants grow either from *seeds*, properly so called, or from *bulbs* or *buds* formed in the axils of their leaves, or out of some part of their cellular system. We may therefore admit a similar explanation of the double fructification of Rhodosperms, namely, that one of their fruits is the analogue of a *seed*, the other of a *bud;* and such is the usual explanation of the difficulty. But there will still remain unanswered the question,—which sort of fruit is the *seed*, and which is the *bud?* In flowering plants nothing would be more easy than to answer such a query, for we know that *seeds* are only formed through the action of stamens and pistils in special assemblages of organs, called flowers. But among cryptogamic plants the floral organs appear in a state so much reduced, or they are so much confounded with organs of nutrition, that it is often difficult to decide upon the true nature of the several parts. And in the present case (of the Rhodosperms) botanists have, at different times, held opposite opinions on the respective value of the *spore* and the *tetraspore*.

" Formerly the *spore* was regarded as the true *seed* (and called " primary fruit"), and the *tetraspore* as a propagulum or bud (and called " secondary fruit"). M. Decaisne originated an opposite hypothesis, alleging that the *tetraspore* was the true analogue of a *seed*, and attributing very inferior importance to the *spore*, denying its reproductive nature in some cases altogether, and in none admitting it to rank higher than a *bud* or

propagulum. This view has been adopted by Agardh, and may be considered the notion commonly held by botanists.

" The *opposite* and older opinion is still held and defended by Areschoug, an able Swedish algologist, and, in this country, by Mr. Thwaites, of Bristol, a most expert and accomplished cryptogamic botanist, and a distinguished physiologist. These authors consider that as the spores are usually formed in special organs or *conceptacles*, accompanied by peculiar transformations and the growth of special tissues, and as the *tetraspores* are commonly dispersed through the substance of unmetamorphosed branches, the *former* have more the character of *special* reproductive bodies than the latter, and should therefore be regarded as the representatives of *seeds*. This reasoning will apply to a considerable number of cases, but not to all, for in many instances we find *tetraspores* formed in special organs, as complex in structure as the conceptacles of the *spores*.

" As far as I am concerned, I have hitherto hesitated to form or express any opinion on this puzzling matter, believing that the evidence was pretty nearly balanced, and that deductions which appear clear when we take in only a few selected cases are considerably weakened when the whole subject comes under review. I could never quite give up the seminal nature of the *spore*, yet, in many cases, I was forced to admit the high organization of the *tetraspore*. And there are cases in which it is difficult to say whether the body be a *spore* or a *tetraspore*. One of these anomalies occurs in *Corallina*, in which genus we have quadripartite *spores* (or *tetraspores*) contained in conceptacles. These bodies are, by their position, strictly analogous to the *spores* of *Polysiphonia*, and yet in their structure they have the character of *tetraspores*. Cases of this kind, and they are by no means isolated, make me very cautious of expressing a decided opinion at present on this question. Meanwhile some arguments, resting partly on analogy, partly on observation, have recently suggested themselves to me in favour of

the claims of the *spore* to be regarded, at least in certain genera, as the analogue of the *seed;* and my present object is chiefly to place on record a slight outline of the argument, proposing, at a future time, to return to the subject, and treat it in the detail that its importance deserves. For, however trivial the discussion of such a question may at first sight appear, much depends on our right solution of it. It is like one of the first steps in a chain of reasoning, the wrong determination of which will vitiate all subsequent inferences. If we have incorrect notions respecting the morphology of these vegetables, all our ideas respecting them will be distorted.

"I shall, on the present occasion, confine myself to some brief remarks on the development of the frond and of the conceptacular fruit in the well-known genus *Polysiphonia.* If we examine a young, growing specimen of any species of this genus, we find that the tips of all its branches terminate in a tuft of dichotomous fibres. The branch consists of a number of cells, placed in a radiating manner like the spokes of a wheel, round a central cavity. Towards the tips of the branches these radiating cells are gradually shorter, and each cell of the last whorl or wheel is prolonged into a dichotomous fibre. This *fibre* never changes its character till it falls away, but the cells (of the branch) below it lengthen and grow wide till they assume their proper form and size. *Growth,* therefore, takes place below the *apical* fibre. Such is the case in the primary branches. When a *new* lateral branch is about to be given off from a primary one, a dichotomous fibre, similar to those at the apex of the old branch, makes its appearance opposite one of the dissepiments of the old branch. Under this fibre a cellular nucleus begins to be formed, which increases in size, and takes the character of one of the branches, new fibres being developed upon it as it acquires complexity. As such fibres are constantly met with on all the *growing* apices while the frond is in process of extension, it is not unnatural to suppose that they are actively concerned in the development they ac-

company ; otherwise why should they be formed with such regularity? They are not peculiar to one species, but are found on the young fronds of all, as well on those from Cape Horn and New Holland, as on the common species of our own coasts. Similar fibres are found on the young parts of other algæ, especially of the *Sporochnoideæ* and *Dictyoteæ*, in the former of which they are evidently very essential organs; and I am of opinion that the monosiphonous ramuli of *Polysiphonia byssoides* and its allies, and of all the *Dasyæ*, are organs of a similar nature, but are in higher development in those plants than in the majority of the *Polysiphoniæ*. Imperfect as they seem to be, I am inclined to regard them as *leaves*, or the analogues of those organs.

"The only argument that occurs to me why we should not regard these fibres as acrogenous leaves is founded on their minute size and imperfect development. But this can be no valid objection to their analogical character. Even among perfect plants, such as Exogens, we often find the leaf reduced to a minute scale, while its place is supplied by a peculiar frondose development of stem, as in the Cacti, and, in a still more striking manner, in the euphorbiaceous genus *Xylophylla*. In this latter the small branches are flattened and green, like leaves, while the true leaves are reduced almost to simple fibres, and are only found on the young branches. Compared with the organization of *Xylophylla*, such leaves are incomparably less perfect than the fibres of *Polysiphoniæ*, as compared with the organization of that genus. Imperfection of development is, therefore, no valid objection to the analogy between the apical fibres and acrogenous leaves; and if this analogy be admitted, we establish the first step in our argument.

"We have in the next place to determine the morphological relation of the ceramiduim or case in which the tuft of spores is contained. This spore-case is, in all the *Rhodomeleæ* and *Chondrieæ*, simply a truncated branch of the frond; a branch diverted from its normal character, and changed into an ovate

or pitcher-shaped hollow body, pierced at the apex, and containing a tuft of spores. That a ceramidium is really a metamorphosed branch is apparent from the inspection of any plant of the family; no phycologist will deny the assertion, so I shall not waste the Academy's time by proving it, but proceed to inquire what metamorphosis has taken place.

"The *ceramidium* makes its appearance, as a young branch does, on the side of an old one; or it is formed but rarely at the apex of the branch. In either case it is at first a little round knob, destitute of apical fibres. This knob gradually swells, but does not greatly lengthen, becomes urceolate or ovate, and is finally pierced at the apex. On opening it we find a tuft of fibres, with their terminal cells converted into pear-shaped spores, attached to a cellular placenta at the base of the spore-case. What metamorphosis have we here? The lengthening of the branch is stopped, and the powers of life concentrated on the elaboration of the contents of the ceramidium. The placenta at the base of the ceramidium is evidently the proper apex of the branch; if this be so, the walls of the ceramidium, as well as the stalked spores within, are probably transformations of the apical fibres. Or we may suppose an introversion of the apex to take place, analogous to what appears to occur in the Fuci; or that, the onward growth of the branch being stopped, owing to the altered condition of the apical fibres (the cause of this altered condition being a fertilization of their cells), the cellular substance continues to develope *laterally* for a time, until it have formed the walls of the conceptacle. Whichever hypothesis we adopt, I think we are warranted in regarding the tuft of spores as the metamorphosed apical fibres.

"I have already endeavoured to show the probability that the apical fibres are the analogues of leaves. If this be admitted, and that it be also admitted that the contents of the ceramidium are apical fibres diverted to another purpose, then we shall have strong analogical evidence in favour of the seminal

character of the spores, for here we arrive at a clear resemblance to the metamorphosis of flowering plants. In flowering plants the *flower* is a truncated branch, and all its parts are metamorphoses of leaves; this flower produces *seeds*. In the algæ of which we speak, the ceramidium (or spore case) is a truncated branch, and its parts are modifications of the apical fibres or supposed leaves; this spore-case produces *spores*. *Seeds* in the first case, and *spores* in the second, are thus formed, so far as we can perceive, under analogous circumstances. It is therefore not unreasonable to infer that the bodies so formed are analogous to each other. The same cannot be said respecting *tetraspores*. One step in the analogy is, however, deficient. We know in what manner the germs of *seeds* are fertilized, but we have yet to learn under what circumstances this alteration in the condition of spores takes place, whether previous to the growth of the *ceramidium*, when the spore may be supposed to exist under the form of a naked ovule, or subsequent to the formation of the ceramidium, and full organization of the spore. I am not prepared with any evidence on this most obscure subject.

"I shall only further remark, as strengthening the analogy derived from the metamorphosis of flowering plants, that in *Polysiphonia* the *antheridia* (or supposed *stamens*) are formed, as the *spores* appear to be, by a metamorphosis of the cells of the apical fibres. In flowering plants we know that stamens and pistils are merely modifications of a common *type*, altered for a special purpose. And here we find that *spore* and *antheridium* have a common origin, each in the apical fibre; but *spores* are produced when the branch is metamorphosed into a conceptacle, and *antheridia* are formed on the fibres of the unchanged branches, and developed externally."

Dr. Allman, in confirmation of Dr. Harvey's views, referred to the fructification the *Charæ*, whose whorls are regarded by

him as analogous to the apical fibres of *Polysiphoniæ*, described by Dr. Harvey.

Rev. Samuel Haughton communicated to the Academy an account of the late Professor Mac Cullagh's lectures on the rotation of a solid body round a fixed point, compiled from notes of his lectures.

The Secretary read a paper by Mr. Henry Henessy, "On the Influence of the Earth's figure on the Distribution of Land and Water at its Surface."

" In a paper, read before the Geological Society of Dublin, on the Changes of the Earth's Figure and Climate, resulting from causes acting at its surface, the author endeavoured to show that certain phenomena, which in some quarters were supposed to be explicable by appealing to such causes, are not at all capable of being so explained. In support of this conclusion it was stated that if, in accordance with the assumptions of the theory considered in the paper alluded to, the earth were originally a solid sphere, and if the ratio of its mean equatorial to its mean polar radius continually increased, the area of dry land at the equator, compared to its area at the poles, would also continually increase.

" To the author this proposition appeared so evident that he did not think its formal proof required to be exhibited. As, however, it subsequently seemed desirable that such a proof should be produced, he has attempted in this paper to fulfil that object.

" Besides proving the proposition in question, the author believes that he has arrived at a new result, which alone would support the views he advocated in the paper already cited.

" 1. If, in accordance with the fundamental assumptions of the theory considered in the paper referred to, the earth